Conspectus Floræ Africæ, ou Enumération des Plantes d'Afrique Par Th. Durand et Hans Schinz. Vol. v. Monocotyledoneæ et Gymnospermeæ. 8vo. Pp. 977. (Bruxelles, 1895.)

IT may, perhaps, be asked why the fifth volume of a work should appear before the fourth and all the preceding ones. Doubtless the authors were influenced thereto by the fact that neither Oliver's "Flora of Tropical Africa," nor Harvey's "Flora Capensis," has reached the groups enumerated in the bulky volume under notice. Certainly this course has the advantage of utility, and will be of great service in the elaboration of the continuation of the works named. As an index to the scattered literature of the subject, the present volume is indeed invaluable. It covers all that may be called African, including the Atlantic islands from Madeira to Tristan d'Acunha, and the islands of the Indian Ocean, from St. Paul and Amsterdam to Mauritius, Madagascar, and Socotra. It is true, the geography of the plants is not worked out all through so fully as Mr. C. B. Clarke has done the Cyperaceæ. For instance, the characteristic grass of Tristan d'Acunha and St. Paul and Amsterdam islands, *Spartina arundinacea*, is only recorded from the former group. In other respects, Mr. Clarke's elaboration of the 800 Cyperaceæ is by far the most complete and thorough part of the volume, though it is blemished by the introduction of a very large number of names of new species without descriptions.

But, leaving all criticism out, this volume will be welcomed alike by horticulturists and botanists; by the former, more especially, because it contains the petaloid monocots, so numerous in South Africa. Synonyms and references to figures in the various illustrated serials add to the usefulness of the enumeration. To give an idea of the extent of this compilation, it may be mentioned that the Liliaceæ include nearly 1100 species, belonging to 67 genera. *Aloe* alone numbers nearly 100 species. The Irideæ are about 700 strong; *Gladiolus* being the largest genus, with 143 species. Orchids also exceed 1000 species, belonging to 74 genera; and 160 species of *Habenaria* are enumerated. Palms are less numerous than might have been expected, considering the comparatively large number in a small group of islands like the Seychelles. Only 63 species are given, which is about a quarter the number inhabiting British India. This is largely due to the genus *Calamus* being represented by only one species in Africa, whereas there are 72 in India.

W. B. H.

Leçons de Chimie. Par H. Gautier et G. Charpy. (Paris: Gauthier-Villars, 1894.)

THE general plan of the second edition of this work does not differ essentially from that of its predecessor. The introductory portion on generalities—dealing with states of aggregation, laws of combination, equivalents and atomic weights, physical and chemical transformation, chemical equilibrium, the velocity of reaction, thermochemistry, &c.—has been recast, and now occupies one-fifth of the volume. In the descriptive portion, which is concerned with inorganic chemistry only, Moissan's work on fluorine, the diamond, and boron has been introduced. It is characteristic of a French text-book that even now it is deemed necessary to print alongside each important atomic equation the corresponding equation based on equivalents. In the same connection it will be somewhat disconcerting to English students to find that "Le poids atomique est égal au poids équivalent pour les éléments suivants: . . . Pour tous les autres éléments, la valeur du poids atomique est double de celle de l'equivalent." In other respects the book is well up to date, and contains much useful information expressed with the clearness and precision for which French text-books are deservedly famed.

J. W. R.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Variation and Specific Stability.

I AM afraid that in my anxiety to compress too long a statement, I did not make the points which I wished to bring forward in the recent discussion at the Royal Society sufficiently clear. I have therefore written out the following summary:—

(1) All organisms vary. That in doing so they obey Quetelet's law was suggested by Mr. Darwin himself more than twenty years ago. He observes (NATURE, September 25, 1873, p 432):—"It is known from the researches of Quetelet . . . that men may be grouped symmetrically about the average with reference to their height. . . . We may presume that this is the usual law of variation in all the parts of every species under ordinary conditions of life."

(2) Prof. George Darwin supplemented this in a following number (October 16, p. 505) with a very lucid account of the principle. In this he says:—"We may assume with some confidence that under normal conditions, the variation of any organ in the same species may be symmetrically grouped about a centre of greatest density."

(3) A well-known illustration is that of a marksman shooting at a target. The distribution of his shots will follow the same law; they will be grouped round a centre of greatest density, which is easily ascertained, as it is the centre of gravity of the circumscribed figure. And on successive trials, if all conditions remain unaltered, the position of the centre will remain the same, though the positions of the shots will be different.

(4) No two individual representatives of a species in nature are exactly alike. All differ in some respect. We may picture the aggregate, however, as grouped with respect to any discriminating character like the shots on the target. Our conception of the species to which they belong is an abstraction which we endeavour to represent in our museums by a specimen which would be placed as near as possible to the centre of greatest density. Such an abstraction we may call the *mean specific form.*

(5) Returning to the case of the target, it is obvious that if some new condition be introduced, such as a wind blowing transversely, every shot will be affected, and the centre of density of the system will be shifted. What is the analogous result when we are dealing with the aggregate of individual organisms representing "a species"?

Natural selection will come into play, to begin with. It may be that some hitherto indifferent variation may be favoured by the new condition. Others will be relatively handicapped, and such a favoured variation will get the upper hand. It is obvious that the result will be to shift the centre of density: the mean specific form will have undergone a corresponding change.

(6) It is probable that so simple a result is not the usual one, and what actually takes place is much more complex.

Mr. Darwin concludes "that organic beings when subjected during several generations to any change whatever in their conditions, tend to vary." ("Variation of Animals and Plants," ii. p. 250.) I infer therefore, and all the facts which have come under my observation confirm it, that a change in the external conditions, otherwise the *environment*, will provoke *some* variation in the organism, which I may call the *stimulated variation.*

(7) It appears to me that from the Lamarckian point of view, the stimulated variation ought to be immediately adaptive. From the Darwinian this is not necessarily the case. It may be either advantageous or, at any rate, indifferent. ("Changed conditions generally induce mere fluctuating variability." Darwin, "Origin," 6th ed. p. 131.) Prof. George Darwin, in the note above cited, traces out the result in the two cases. In the former case, "with continual intercrossing," the new variation will get the upper hand, and the centre of density will be shifted; in the latter it will, by continuous "weeding out," be, after a temporary displacement, eventually restored.

(8) This leads to the consideration of the *stability* of the mean specific form. Some species seem to yield pretty rapidly, though with an appreciable *inertia*, to the influence of changed condi-

tions; others seem almost indefinitely to resist it. Probably, however, even the most intractable cases may eventually be broken down. This stability has been largely used as an adverse argument to organic evolution generally. Prof. Decaisne was much influenced by it, and stoutly maintained not merely the permanent stability of species but even of varieties. His experiments on the seminal reproduction of cultivated pears, which extended necessarily over a long series of years, was, as might be expected, adverse to varietal stability, and left him in a position from which he never extricated himself.

The existence of specific stability is, however, undoubted. Mr. Carruthers devoted his address to Section D of the British Association in 1886 to a useful summary of the ascertained facts on the subject. He laid particular stress on the well-known data, which are illustrated in the Kew Museums, as to the flora of Egypt. This, 4000 years ago, was composed of species which differed apparently in no particular from their present living representatives.

An even more striking illustration is afforded by the history of standards of weight. From Prof. Ridgway's researches it appears that these were originally based on seeds ("Origin of Currency and Weight Standard"). He finds (p. 182) that "the Troy grain is nothing more than the barley-corn." Further, "in 1280 (8 Edward I.) the penny was to weigh 24 grains, which . . . were as much as . . . 32 grains of wheat" (p. 180). The ratio still obtains. "In September, 1887, I placed in the opposite scales of a balance 32 grains of wheat 'dry and taken from the midst of the ear,' and 24 grains of barley taken from ricks of ears grown in the same field at Fen Ditton, near Cambridge, and I thrice repeated the experiment; each time they balanced so evenly that a half-grain weight turned the scale." Further, he found that "practically 4 wheat grains = 3 Troy grains" (p. 182). The same fact of stability can be illustrated from other seeds used as standards of weight. No working naturalist will then be disposed to very much quarrel with the conclusion arrived at by Mr. Carruthers, that "species must be dealt with as fixed quantities."

(9) Notwithstanding this, the counterfact remains that it is doubtful if there is any species the stability of which cannot be broken down by appropriate methods. I, however, prefer to say not that a species is "fixed," but that its mean specific form is appreciably stable under permanently uniform conditions. And this is probably what Mr. Carruthers really meant.

The interesting question then arises, how is this stability brought about?

(10) The principle of stability seems to me scarcely to have received the attention it deserves. Mr. Darwin has, I think, never expressly stated it, though he was evidently well acquainted with it, and has supplied some explanation of its causes.

In dealing with the facts referred to above as to the Egyptian flora, he simply says: "In Egypt, during the last several thousand years, the conditions of life, as far as we know, have remained absolutely uniform." ("Origin," 6th ed. p. 169.) The implication, though he does not actually say so, is that under such circumstances evolution is in abeyance. If it were not, it seems to me that we must admit Nägeli's "innate tendency towards progressive and more perfect development." Mr. Darwin states, however, the correlative principle with greater definiteness. "The influence of changed conditions accumulates, so that no effect is produced on a species until it has been exposed during several generations to continued cultivation or domestication." ("Animals and Plants under Domestication," ii. 261.) I have no doubt that this is true, and I entirely agree with De Vilmorin, cited by Mr. Darwin (*l.c.* p 262) that "the first step is to get the plant to vary in any manner whatever, . . . for *the fixed character of the species* being once broken, the desired variation will sooner or later appear."

More or less close interbreeding will, it is well known, tend to stability. Mr. Darwin tells us: "After a dozen generations of self-fertilisation, it is probable that the new variety would remain constant, even if grown under somewhat different conditions." ("Cross and Self-fertilisation," p. 460.) The same fact is illustrated "by the survival during at least half a century of the same varieties of the common pea and the sweet-pea." (Darwin, *l.c.* p. 39.) Self-fertilisation has the positive advantage that it "assures the production of a large supply of seeds." (Darwin, *l.c.* p. 41.) The price ultimately paid is, however, probably a very heavy one—that of extinction. (*Cf.* Darwin, "Life," iii. 276.)

(11) The case of wheat is peculiarly interesting. It is not worth while pressing the evidence too far. But it may be remarked that wheat has by no means lost its power of variability. Mr. Carruthers remarks:—"The improved varieties of our cereals now under general cultivation have been obtained almost entirely from the selection of individual plants." (*Journ. R. Agric. Soc.* 4th ser., iv. p. 684.) Mr. Darwin ("Animals and Plants," i. 318) quotes the authorities for the statement that the wheat cultivated by the lake-dwellers in Switzerland had very small grains. Still we may admit that for a very long period the average wheat grain has not appreciably varied. This appears to me to prove nothing more than that wheat has got into a stable state, which I should attribute to the average uniformity of the conditions under which it has been cultivated. I shall not press the question of its self-fertilisation, which is generally accepted, but which Darwin (*l.c.* i. 316) thinks not invariable. Somewhere he has stated, though I cannot put my hand on the reference, that the size of the grain is disadvantageous.

If so, this would be unfavourable to its increase. De Candolle ("L'origine des plantes cultivées," p. 370) regards the original source of the wheat-plant as extinct, and suggests that the attractiveness of its grains for birds may have been the cause. At any rate it appears certain that but for cultivation it would now entirely disappear. The conclusion seems to be that mankind has brought it up to a certain standard which was sufficient for its purpose, and has not been compelled to carry it further by unconscious selection.

The case of maize, which has also been quoted, is much less to the point. It is now pretty certain that its origin is to be found in a form still existing in which the grains are only two-rowed. But maize, under existing cultivation, proves to be extremely variable.

(12) It appears to me that an important light is thrown upon what I may be allowed to term the *stability problem* by the remarkable investigation recently presented to the Royal Society by Prof. Weldon. It is one which I am persuaded would have given Mr. Darwin peculiar pleasure.

Prof. Weldon measures very carefully some organ in each individual of a given population. The measurements are plotted out along a base line; the number of individuals in which each measurement occurs are represented proportionately by lines drawn perpendicular to the base line. The summits when connected form a curve, which is termed a *frequency curve*. For a discussion of the general principle and of the interpretation of such curves reference must be made to the able investigation for which biologists are indebted to Prof. Karl Pearson (*Phil. Trans.* 1894, A., pp. 71–110).

Prof. Karl Pearson (p. 72) terms "a frequency curve which for practical purposes can be represented by the error curve," a normal curve. Such a normal curve, I apprehend, is only another way of representing the result of Quetelet's law, which, as I stated above, there was reason to suppose was the ordinary law of variation. For he adds: "When a series of measurements gives rise to a normal curve, we may probably assume something like a stable condition; there is production and destruction impartially round the mean."

By applying the method to a crab from Plymouth Sound, Prof. Weldon has been able to throw light, it seems to me for the first time, on a most important factor in the stability problem.

"About 7000 females . . . were chosen (at random, except as regards their size), and two dimensions were measured in each. The results were then compared with those of the corresponding measurements, made upon a sample of 1000 adult females from the same locality."

The result was to show that "selective destruction" takes place in early life amongst individuals, which deviate from the "mean specific form." Prof. Weldon arrives at the conclusion "that the position of minimum destruction should be sensibly coincident with the mean of the whole system," is a condition which "may be expected to hold for a large number of species, which are sensibly in equilibrium with their present surroundings, so that their mean character is sensibly the best." The actual statistical demonstration of this fact, in my opinion, deserves to rank amongst the most remarkable achievements in connection with the theory of evolution.

(13) When the frequency-curve is abnormal, Prof. Karl Pearson infers "the pressure at a given time of some particular form of natural selection." The mathematician may in this case be able

to render assistance to the biologist of supreme importance. "The amount and direction of the abnormality will be indicated if this [abnormal] frequency-curve can be split up into normal curves." Analysis would in this way give us information which we could perhaps not even guess at.

I agree with Prof. Karl Pearson that "resolution into two" will be sufficient. The stress of natural selection at any moment must always be between the best and the next best.

In tolerably simple cases I have no doubt that the result of Prof. Karl Pearson's labours will be to throw great light on the matter. In more complicated ones we must look for some disappointment in view of "the great variety of solutions which may be suggested" (p. 106). And the tentative discovery of "component normal curves" seems likely to be fallacious (p. 90).

(14) I think it is important to insist that the importance of Prof. Weldon's present results has reference to the stability problem. He is fully aware of this fact when he says that "they cannot be expected to hold in cases of rapid change such as those induced artificially by selection under domestication, or naturally by rapid migration or other phenomena resulting in a rapid change of environment." These will lead to abnormal frequency curves.

(15) A few remaining points in Prof. Weldon's paper deserve some remarks.

I entirely agree with him in minimising the value of "sports" in evolution. As against Nägeli and his followers, I see no ground for believing in any innate progressive tendency in organisms. When the organism is in stable relation with its environment it will continue so indefinitely. That is the conclusion I deduce from the flora of Egypt, and other facts which have been cited of the same kind. Prof. Weldon seems to me to have supplied this position with a most important proof by establishing the "selective destruction" of variations aberrant from the mean specific form. When the environment varies, stability is destroyed; but it will be ultimately reestablished, though with a different centre, by the operation of natural selection. The result is that the organism has undergone some permanent degree of change. As I conceive the process, it is one of continuous adjustment of "slight" variations on one side and the other. But it is important to keep in view that variation in the environment stimulates the variation in the organism which supplies the ultimate material for adjustment. That the amount of the adjustment at any moment is slight is not incompatible with its amounting to almost anything we like in the aggregate, if sufficient time be allowed. We might as well deny that a curve can be built up from its infinitesimal elements.

The value in this respect of sports may be easily overrated. It appears to me that generally, so to speak, they attempt too much and overshoot the mark. The improbability of a casual sport being exactly what is wanted to bring the organism into an advantageous relation to the environment at any particular juncture seems to me very great. That such a thing may occur is not denied, but it can hardly be more than a "fluke."

(16) This is confirmed by the fact that in the vegetable kingdom sports are rare, and they seem to have little power of holding their own in competition. I instanced the cases of the occurrence of copper-coloured and laciniate foliage in many trees, as well as the occurrence of varieties with weeping and fastigiate habit. It is well known that to some extent these are perpetuated by seed. But the existence of such forms would undoubtedly be transient if it were not for their perpetuation as curiosities under cultivation.

(17) In museums it is usual to attempt the representation of the mean specific form. The association of strikingly aberrant specimens is interesting and often suggestive. But I do not see that they illustrate more than the possibilities of variation and the fact that it may be discontinuous.

(18) I placed upon the table plants of the feral type and of a recent cultivated form of *Cineraria cruenta* from the Canaries. The difference in habit and in the form and colour of the flowers was enormous. This has undoubtedly been brought about by human selection. As far as is known it has been accomplished by the gradual accumulation of small variations. The horticulturist has not troubled himself about the foliage, which, though more luxuriant, has remained practically unchanged. But it must not be assumed that it is unchangeable. In the case of the Chinese primrose, the feral form from north-west China, for which we are indebted to Dr. Henry, has palmatifid leaves rounded in outline; but the distal lobes are occasionally lengthened out. The horticulturist, as a matter of fancy, working on this, has now split the type into two races, one of which has palmatifid and the other pinnatifid leaves. Many botanists would, undoubtedly, if they did not know their history, assign to such a different specific rank.

(19) I am not sure that I quite understand Prof. Weldon when he says that "the statistical method is the only one at present obvious by which [the Darwinian] hypothesis can be experimentally checked." In the first place, I should myself hardly call it experimental at all. In the next place, though I think it will throw important light on the stability problem, in the important cases where evolution is actually taking place, the mathematical analysis appears to me to be beset with very great difficulties. We must not, therefore, expect too much from it.

(20) On the other hand, museums, as at present organised, do not help very much the study of evolution. In the case of plants, I doubt if herbaria will ever be able to present material in a sufficiently compendious or complete form to be of much use. The study, however, of extensive series of a few species of insects ranging over the whole of a large geographical area, such as Mr. Elwes has brought together in the case of butterflies, must, it seems to me, afford most important material for future discussion. W. T. THISELTON-DYER.

Royal Gardens, Kew, March 10.

Do Plants Assimilate Argon?

It is a well-known fact that some plants are able to assimilate nitrogen from the atmosphere and form compounds. Now, as argon cannot be induced—at least up to now—by any known process of inorganic chemistry or physical science to enter into a combination with one or more of the known elements, it occurred to me whether that peculiar power which produced the cell is not able to form combinations with argon. The experiment to grow suitable plants in an atmosphere of pure argon, or argon mixed with pure oxygen, on a bed of pure sand, &c., would easily settle the question.

If this experiment has not yet been made, perhaps you will find space in your paper for the above few lines.

Essen-Ruhr, March 6. E. BLASS.

THE first thing is obviously to find whether there is any argon in a nitrogenous vegetable; and experiments are now nearly completed in my laboratory to see if nitrogen obtained from peas contains any argon. Similar experiments are being made with nitrogen from mice. In a few days I shall know the results. But this is, of course, on the assumption that the process which liberates nitrogen also liberates argon; and it is by no means certain. It should be remembered that argon and nitrogen have absolutely no similarity, and that their occurrence together in air is a pure accident, due to the inertness of both.

March 10. W. RAMSAY.

The Measurement of Pressures in Guns.

In a paper "On Methods that have been adopted for Measuring Pressures in the Bores of Guns" (Report of the British Association, 1894), Captain Sir A. Noble has remarked that it seems to him "that there is no method so satisfactory, despite its attendant labour, as that of *making the projectile write its own story*" (p. 540). That might be sufficient for *smooth* bore guns, where there was little or no friction in the bore, but it is quite unsatisfactory when applied to *rifled* guns, and especially B.L. guns.

The two methods of experimenting now in use employ the *pressure gauge* and the *chronograph*, both of which we will, for this occasion, suppose perfectly accurate. The pressure gauge measures directly the pressure of the powder gas P, the quantity wanted. The chronograph will measure P, the same pressure of the powder gas, *minus* F, the resistance offered to the motion of the shot by the rifling, the friction of the driving-band, &c., = P − F, where F is often very great. The difficulty is to see how these two different processes can confirm one another, as F is unknown and of great importance.

The only satisfactory method of determining the maximum pressure of powder gas at any point in the bore of a *rifled* gun is to measure it *directly*, by the pressure gauge, which requires many precautions to be taken, or by some other more simple method. F. BASHFORTH.

Horncastle, March 9.